

Comments on "Formulation and Testing of a Program for the Objective Assembly of Meteorological Satellite Cloud Observations"

BRUCE F. WATSON

Systems Research Division, Honeywell Inc., Minneapolis, Minn.

In their recent article, Messrs. Nagle, Clark, Holl, and Riegel [1] make note of what they consider to be a number of significant discrepancies between their probability-of-cloudiness maps and observations. Many of these discrepancies do not exist in fact, and it is the purpose of this note to show why. It will be the conclusion that their method of determining probability-of-cloudiness charts works much better than they believed.

It is fortuitous for the discussion that I worked in great detail on the global cloud systems which included the same time period [2] which the authors used to test their method. Hence, I am quite familiar with the cloud systems and sequence of events mentioned by Messrs. Nagle et al.

The principal reason for the apparent discrepancies lies in the definition of significant clouds. The authors used as significant clouds "organized cloud patterns of 80 percent or greater coverage," with exclusions for areas less than

80 percent "when they were integral components of organized cloud systems." This definition unfortunately does not take into account the physical basis of cloud systems.

I define a major cloud band system essentially as one which (except for tropical systems) is born in the southwesterly flow ahead of a developing trough line in the middle and/or upper troposphere (usually apparent at 500 mb.) and which exists as a recognizable, organized entity for several days to 2 weeks. Such systems are composed of multi-layered clouds with regions of convective clouds. The system forms itself in long north-south bands largely as a result of wind shear, and usually has a vortex at its poleward end. At about 20° latitude, these bands become oriented east-west as they dip below the westerlies. In these tropical regions they may develop various vortex or quasi-vortex configurations, but usually eventually die from lack of sufficient convergence.

In the mid-latitudes, such a major cloud band may develop a wave somewhere along its extent in the classical Norwegian sense, in which case the new cloud system is actually a branch of the major cloud system.

Because of the lack of large-scale cloud organization in the Tropics, with some notable exceptions, endurance and extent of the feature are perhaps the best workable means of defining a significant cloud system in this region.

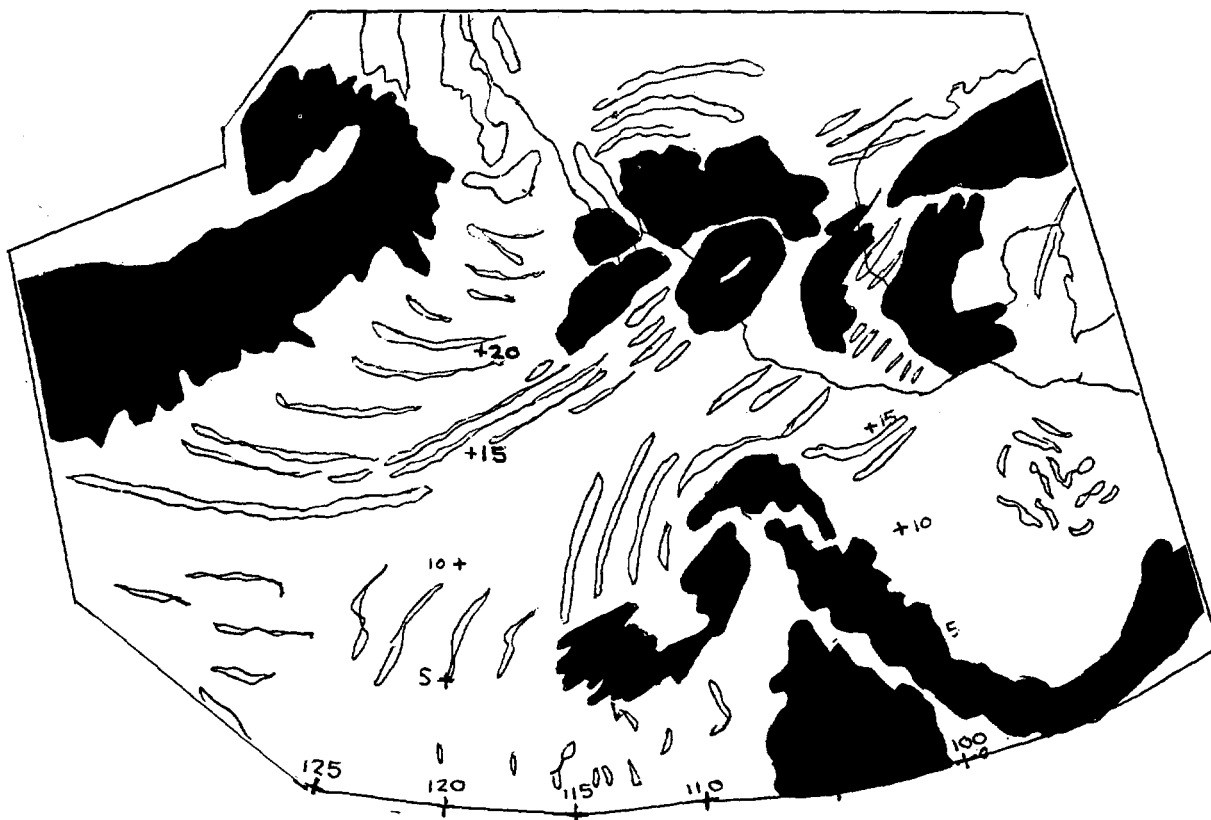


FIGURE 1.—Detailed nephanalysis for 2130 GMT, February 2, 1965. Significant cloud systems are shaded.

Using this concept of definition, I compared my neph-analysis with those presented by the authors and the probability-of-cloudiness charts (diagnostic charts). The results are quite interesting.

On page 182, the authors note a "discrepancy" between the diagnosed and the observed clouds south of Iceland on 1500 GMT February 2 (fig. 10b in their paper). They are quite correct in rejecting this cloud layer since they observed only stratocumulus on the videograph. Had they initially rejected these clouds, which form *behind* the trough as a result of the flow of cold air over warm water, this discrepancy would not have occurred at all. These stratocumulus, stratus, or cumulus clouds are easily identified on the satellite cloud pictures. Their rejection in the nephanalysis of significant clouds would help greatly toward eliminating discrepancies.

More important because of their definition, the authors missed identifying a rather significant cloud system in the nephanalysis they used in figure 12a and as a result had an apparent discrepancy that in fact did not exist at all. In my nephanalysis, there was an extensive cloud system in practically the exact area where their method diagnosed one to exist! This is shown in my figure 1. This system existed in the generation region for many days, and its shape suggests cyclonic flow. The authors' method succeeded where they thought it had failed.

Further, my nephanalysis does show a breakup of the clouds over the Texas-Mexican border and eastward, nearly as they show on their probability-of-analysis chart! The nephanalysis they used did not show this. Again their method succeeded.

A cloud system which the authors successfully diagnosed with their method, but were surprised to find agreement with, was one which appeared over Mexico. This is the system over the Baja California region which they discuss on page 182 and depict in figure 12. This is also shown in my figure 1. This system was associated with a trough that had advanced across the Pacific, and which became quite weak as it crossed the Rocky Mountains. The clouds associated with this trough became quite weak north of Mexico, while the clouds maintained some identity while crossing Mexico. The system over Mexico, associated with this system, later appeared as a large cloud mass over the western Gulf and Texas coast on February 3, 1965.

Comparisons that I made with the other figures in the article verify that the authors have very definitely diagnosed major cloud band systems more adequately than they thought. They simply used an inadequate basis for defining significant major cloud band systems. I do think they would find it quite fascinating to use some of the Nimbus, ESSA satellite, and ATS data in testing their method in connection with their probability-of-cloudiness method.

REFERENCES

1. R. E. Nagle, J. R. Clark, M. M. Holl, and C. A. Riegel, "Formulation and Testing of a Program for the Objective Assembly of Meteorological Satellite Cloud Observations," *Monthly Weather Review*, vol. 95, No. 4, Apr. 1967, pp. 171-187.
2. B. F. Watson, "Global Cloud Systems," submitted for publication, 1966.

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Reply

ROLAND E. NAGLE

Meteorology International Inc., Monterey, Calif.

We wish to thank Mr. Watson for his interest in our paper. The essence of his comments concerns the adequacy of our definition and analyses of regions of synoptically significant layer cloudiness. Mr. Watson's definition is undoubtedly more precise (synoptically) than ours and its use in governing the analyses might have produced better correspondences between the observed and diagnosed cloudiness. However, his suggested procedure suffers from the same inherent limitations as ours in that it requires the subjective interpretation of the cloud patterns viewed in the videographs. This points out the need for an automated method of delineating areas of synoptically significant layer cloudiness. Recent developments indicate that such a procedure is now feasible and that, if desired, the cloud information could be assimilated into the Program in a more objective manner.

In this regard, Fritz [1] has shown that cloud albedo is a function of droplet size-and-number distribution, geometric thickness, and sun zenith angle; therefore, synoptically significant layer cloudiness, having a relatively high liquid water content, should also have a high albedo. This has been subjectively confirmed, as evidenced by the bright-appearing cloud bands viewed in the satellite videographs. Brightness is not a necessary and sufficient condition for delineating such cloud areas, as clouds which are not associated with large-scale lifting may also be highly reflective (for example, coastal stratus and stratocumulus clouds). However, the clouds of interest are also usually characterized by their great depth and therefore by their relatively cold tops. Cloud-top temperatures could be used as a further distinguishing criterion for excluding clouds which are not associated with large-scale lifting.

Quantitative measures of both cloud brightness and cloud-top temperatures can be readily obtained from radiometer measurements in appropriate spectral regions [2]. The use of these data in the current context would require the derivation of suitable functional relationships among cloud-top temperatures, cloud albedo, and the occurrence of layer cloudiness. The feasibility of this